

With Compliments of
F. N. Otis

URETHRISMUS

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CHRONIC SPASMODIC STRICTURE.

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CLINICAL PROFESSOR OF GENITO-URINARY DISEASES IN THE COLLEGE OF PHYSICIANS AND
SURGEONS, NEW YORK; SURGEON TO CHARITY HOSPITAL, MEMBER OF THE
BRITISH MEDICAL ASSOCIATION, ETC.

(REPRINTED FROM THE HOSPITAL GAZETTE, APRIL 19TH, 1879.)



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Surgeon to Charity Hospital.

In the February number of Dr. Brown Sequard's *Archives of Medical Science*, for the year 1873, a case was reported by me, in which repeated retentions of the urine had occurred, followed after a time, by habitual incontinence, and finally by perineal abscess and urinary fistula; thus presenting what would, ordinarily, have been attributed to the results of close, deep organic stricture alone. The sequel proved, however, that there was no deep stricture; that the retention, etc., was caused by persistent spasmodic closure of the urethra by the compressor muscles. The complete and permanent relief that followed division of a contracted meatus urinarius, led to the inference that the spasm of the compressor muscles was of reflex origin, and that the true point of irritation was at the urethral orifice.

Similar cases were reported by me, in a paper read before the New York Academy of Medicine, in Feb., 1873, and again still other cases, 6 in number, in an article on Spasmodic Stricture, published in the *Archives of Dermatology*, in Feb., 1875.

In order to recall the important lesson taught by these cases, and to show exactly on what grounds the claim of spasmodic stricture, due to irritation reflected from the anterior part of the urethra, was then based, I now take the liberty of quoting, entire, the first of the six cases there presented.*

Case 1. "J. W., frontiersman, aged 45, came under my notice Nov. 1874, with a history of first gonorrhœa, twenty years previously, and several subsequent attacks. Five years after, began to have diffi-

* (Read before the New York Dermatological Society, Feb. 9th, 1875, published in the *Archives of Dermatology*, Vol. 1. No. 3. Article on Spasmodic Urethral Stricture.

culty in passing his urine. Stream grew gradually smaller, until following a debauch, he had complete retention of urine, and was obliged to seek relief at a neighboring military post. After thirty-six hours suffering, he was relieved, through the passage of a very small, flexible catheter, by the hands of the post surgeon. Subsequently to this, he submitted to treatment by dilatation for several months. He then learned to pass No. 12, English, soft bougie. From neglect he has had some half a dozen attacks of retention during the past year.

At last, only the smallest instrument could be passed by the military surgeon, and he was advised to go East and have a radical operation performed, as 'there were no instruments at the post suitable to operate on so small a stricture.' The habit of the patient, for a long time, has been to pass his water very frequently during the day, in a fine, irregular stream, and several times during the night.

Physical examination. Is of large stature, looking like a strong man who had endured much exposure and hardship. Made his water, in my presence, in fine short jets and dribbling.

Circumference of the penis $3\frac{1}{2}$ inches, size of meatus 23 f.; No. 23 f., steel sound passed easily through a very sensitive urethra, to the bulbo-membranous junction, where it was arrested. Bougies in gradually decreasing sizes were introduced, until finally No. 12 f., passed into the bladder, closely hugged in the deep urethra. Allowing it to remain for a few moments. I found it free. I then withdrew it, divided the contracted meatus and stricture for nearly half an inch back, and passed 34 f., solid steel sound, slowly down to the bulbo membranous junction, when it *slipped by its own weight, into the bladder.*

After the withdrawal of the sound, the patient passed his water in a full large stream. From this moment he had no farther trouble in urination, passing his water at intervals of from six to eight hours during the day, and not at all at night, for the week subsequent to the operation, when he left for his home, in the far West, apparently well in every respect." This, and five similar cases, were claimed to be types of a large class, where all the usually recognised evidences of deep organic stricture, might exist for a long period and yet no

deep seated organic stricture be present; and that this apparent stricture was purely spasmodic, the result of reflex irritation from an anterior stricture, often of large calibre. It was then also claimed that "*the presence of the slightest contraction at any point, may be accepted as capable of producing reflex irritation, which may result in a spasmodic stricture, which shall possess all the recognized characteristics of deep organic stricture.*" And further "*that no reliable examination of the deeper urethra can ever be made while a stricture or even an erosion is present in the anterior part of the canal.*" The very natural inference to be drawn from these cases and views, if they could be shown to be correct, was that a very considerable proportion of the strictures accepted and treated as organic, were probably not true stricture. That, to treat deep stricture without first seeking and if found, removing as far as possible all sources of irritation shown capable of producing spasmodic action, exposed the surgeon to the danger of instituting surgical procedures, more or less grave, at points where no organic disease existed: thus subjecting the patient to unnecessary peril, besides useless pain and annoyance and subsequent damage of various kinds. For the purpose of still further calling attention to the danger and grave consequences of mistaking spasmodic for organic stricture. I published through the ARCHIVES OF CLINICAL SURGERY of Dec. 1876 a striking example of "*Spasmodic stricture of seventeen years duration, causing frequent retentions and also incontinence of urine, cured by division of a contracted meatus urinarius, combined with overdistention of the membranous urethra.*"

The importance of this case, when considered in conjunction with the cases and views previously cited, appeared to me to warrant the expectation that it would have been made the subject of public comment,—perhaps of discussion,—in the societies and journals, especially as it was well known that there were men of prominence in the profession, both here and abroad, who practically and vehemently denied the existence of spasmodic stricture.*

* Mr. Erichsen, the distinguished English surgeon, says on page III4 of his "Science of Art and Surgery:" "Surgeons, disregarding the evidence of their own senses, and being led away by imperfect anatomical examinations have denied the possibility of the spasm of this canal."

I confess myself to have been curious to know how the facts stated, would be explained, without an acknowledgment of the spasmodic influence. Up to a recent [date, however—quite three years from the report of the last case mentioned—no public notice had been taken of the matter. The formidable array of what appeared to me indisputable proofs—facts—had, I began to hope, been quietly accepted, and that the danger of under-estimating the value of the spasmodic element, in the diagnosis and treatment of urethral stricture, was over for all fair-minded and intelligent surgeons. This view of the situation received a rude shock on the perusal, in THE HOSPITAL GAZETTE of Feb. 13th, 1879, of a clinical lecture on “Inflammatory and Spasmodic Stricture,” by Dr. Henry B. Sands. In this lecture, after a specious argument, which tended to obscure the importance of spasmodic stricture, by mixing it up with the inflammatory swelling of mucous membrane, † he then presented a summary of the muscular surroundings of the urethra, in which he described the membranous portion as taught by Hancock and Kolliker, viz., “The membranous division of the urethra is surrounded by a stratum of plain muscle about one *mm.* in thickness, the fibres being for the most part circular. Externally to this layer is found a considerable quantity of striated muscle constituting the compressor urethræ, and capable of forcible contraction.” After thus showing the condition at this point, to be one most capable of, and favorable for, spasmodic action, he goes on to say that, although this is true, “in my(his) judgment purely spasmodic urethral stricture is seldom met with,” and further on, that he believes that a retention of urine from such causes is extremely rare, and that he is extremely doubtful whether this muscle (the compressor urethræ) can contract with sufficient force to prevent the introduction of a catheter properly directed, and that, as far as his personal experience goes, he has yet to meet with a single instance of purely spasmodic stricture. This is Dr. Sands’ individual opinion and

† Sir Henry Thompson, in his latest work on “Diseases of the Urinary Organs” (1876), page 30, says of what Dr. Sands calls *Inflammatory Strictures*, “If you consent to call this condition stricture, you might as well say that the throat is strictured when it is inflamed and the tonsils swollen.”

experience. He "will not affirm that such a form of stricture never exists, as a few examples of this kind have been recorded by competent observers." As to what constitutes a *competent* observer, or who those competent observers are, or were, we are not informed. Dr. Sands then goes on to break a lance against the theory of reflex action. He opens with a misstatement in regard to the origin of the theory, and introduces, with scant courtesy, the distinguished French surgeon, M. Verneuil, to whom he erroneously attributes its discovery. (An error which I shall take an opportunity to correct a little farther on). He then states, *par parenthesis*, that prior to Verneuil's invention of the reflex theory [1866] there was abundant evidence, (acquired by post-mortem examinations,) to prove that the large proportion of cases of organic stricture was situated in the bulbous portion of the urethra, and adduces, in proof of the correctness of this evidence, twelve specimens which he has seen in the New York Hospital. This apparently to prove that all strictures situated in the bulbo membranous region were not the result of reflex action. He then attacks the distinguished French surgeon. He states that the views of M. Verneuil and his pupil, M. Folet, concerning spasmodic stricture, "have not been adopted in his native country." These views, presented by M. Verneuil to the Anatomical Society of Paris in 1866, were briefly as follows, viz., that a large proportion of what are commonly regarded as deep-seated organic strictures were simply spasmodic contractions of the compressor urethræ muscles, due to reflex irritation, transmitted from organic contractions of the anterior portion of the canal. These false strictures were always located in the membranous urethra. M. Verneuil's views were corroborated by citation of cases which had occurred in his experience. A year subsequently, M. Folet made the views of M. Verneuil the subject of an elaborate paper, published in *Archiv. Generales* in 1867, in which he recorded ten cases treated in M. Verneuil's service in the Lariboisiere hospital during seven months, *only one of which was thought to have been organic stricture*, while in nine the apparent deep stricture *was shown to be due to the influence of a stricture in the spongy portion of the urethra.*

But, says Dr. Sands, two things are evident in reading M. Folet's paper. First, that the writer "is unduly desirous of defending a favorite theory." Second, that he "*had mistaken the triangular ligament for a muscular spasm.*" Verneuil, Chief of French Surgeons of to-day, and Folet, his pupil, who had written the elaborate article for the *Archives Generales*, have mistaken the triangular ligament for a muscular spasm! Prof. Sands gravely asserts this. A moment's reflection will assure us that the Professor has fallen into the fault of which he accuses M. Folet, that is, of being unduly anxious to defend a favorite theory. In his haste to do so he evidently overlooks the fact, that, according to M. Verneuil and M. Folet, the obstruction which Prof. Sands attributes to the triangular ligament *disappears promptly and permanently on removal of the anterior strictures, previously located.* Surely Dr. Sands would not now insist that it is the triangular ligament, which (after affording satisfactory evidence of its spasmodic character, to presumably honest and capable observers) is caused to disappear permanently, by division of a stricture in the penile urethra.

Prof. Sands, who had already indicated the value, in his opinion, to be attached to post mortem examination of strictures, and evidently inclined to discredit the results of researches during life, invites our incredulity, while he states the position of the great French surgeon. Thus, "now Verneuil, *who appears never to have examined strictures by dissection,*" asserted that "*deep seated organic strictures, so far from being common, were extremely rare,*" and that "*in the immense majority of cases supposed to be of this nature, the real stricture would be found in the penile portion of the urethra, the contraction of deeper segment being due to a reflex spasmodic action of the compressor urethræ muscle.*" Prof. Sands would have us infer then, that, if M. Verneuil had but dissected his patients, he would have discovered the triangular ligament and thus have been saved the ignominy of such a mistake, as to suppose a spasm had existed during life.

Prof. Sands having thus demolished M. Verneuil and M. Folet, then proceeds to Prof. Otis. He says, "So far as I am aware, the views of Verneuil, and his pupil concerning spasmodic stricture, have

not been adopted in his native country; but I have thought it proper to direct your attention to them, because they were presented here, as a surgical novelty, by Prof. Otis, in 1875, and urged by him as a plea for the performance of operations which I believe to be dangerous and unwarrantable."

To those who have read the earlier part of this article, it will be unnecessary to say, that my views on reflex action were utilized as early as 1873, and that they were not the views of M. Verneuil, nor presented by me as a surgical novelty in 1875, I will prove by quotation from my work on stricture of the Male Urethra ; its Radical Cure;* (published over 8 months previous to the appearance of Prof. Sands article) page 304. (Note. "The results of my earlier observations on the influence of slight contraction of the urethra, in producing various forms of reflex troubles, were first published in Dr. Brown-Sequard's Archives of Medicine in 1873. Since that date, I have, in published cases and in reports to societies, claimed a credit for originality, in the discovery of a direct influence exerted by slight urethral contraction, in producing varied and grave disturbances throughout the genitourinary tract and even in certain instances extending to distant parts of the entire economy. Within a few weeks, however, (May 1878) a careful search through the published writings of M. Civiale, of Paris, (made at my suggestion by my accomplished friend Dr. M. J. DeRosset, of New York) I have found my claims to priority in this matter, to be without foundation. Now, while I claim my own published views and observations, prior to this date, to have been original with myself, I hasten to concede the honor of priority, in this field, to the distinguished French surgeon to whom it fairly belongs. The following quotations are from M. Civiale's *Traite Pratique des Maladies Genito-Urinaires* 2 (E) Paris, 1850.

At page 45, et. seq. of this work, M. Civiale writes thus : "*Independent of its local sensitiveness, the urethra possesses another kind which may be termed sympathetic* * * * When this sensitiveness is aggravated, it may awaken sympathetic response *in every organ and function of the body.* * * In many cases sympathetic (reflex) phenomena were manifest in the lower extremities, particularly in the soles of the feet."

Again, at page 354, *et seq.* "It is not rare to observe that slight encroachments upon the urethral calibre, induce marked difficulty in micturition ; those at the meatus having this effect not less than those located farther in."

Again, at page 160. "Strictures seldom exist for a long time without exciting a series of disorders of the genito-urinary functions, and, consecutively, in remote parts of the body. * * * Among these, gleet, retention of urine, difficult micturition. * * * That which has struck me forcibly in dividing a meatus, only slightly contracted, is the sudden and complete change effected in the general condition of the patient. The constriction, which seemed hardly to impede the flow of urine, is no sooner divided than all morbid symptoms vanish : *the urethral walls which were rigid, hard, and inelastic*, immediately recover their normal condition; the bougie, which at first passed only with difficulty and pain, slips into the bladder with ease, and in five or six days the slight incision in the meatus heals perfectly, and the patient finds himself in a state so satisfactory, that it would be incredible, but for the fact that the instances are again and again repeated. An effect so prompt through means of which the significance is plain, shows that *the slightest obstruction in the urethra is able to produce the gravest symptoms local and general.*"

This then effectually disposes of the statement of Dr. Sands, that the reflex views of M. Verneuil were introduced here, by me, as a surgical novelty in 1875, by proof that the views were my own, and introduced in 1873. It proves also, what is much more important viz : That the theory of reflex action applied, or according to Dr. Sands, "*misapplied*," to urethral difficulties, was first advanced by M. Civiale, one of the most distinguished surgeons of his time, not of France alone, but of the world, and supported with all the force of his position, his personal character, his personal experience.

Sixteen years after, M. Verneuil, the surgeon who, more nearly than any other, occupied and still occupies the commanding position of M. Civiale, repeats and supports this reflex theory, before the Anatomical Society of Paris. M. Folet confirms it in the Archives Gen-

erales a year after, and M. Cornillon again brings proof of its value and truth in 1870, yet Dr. Sands brings, as his only argument against the reflex theory, the statement that, in his opinion, these views of M. Verneuil "rest on a very slender foundation" * * "and are not accepted in his own country." They would not even have been worthy of his attention, but that, not alone were they presented here as a surgical novelty, by Prof. Otis in 1875, but were "urged by him as a plea for the performance of operations which" he says, "I believe to be dangerous and unwarrantable." "It was held," (by Prof. Otis) he further says, "that as a rule what surgeons generally regarded and treated as deep-seated organic stricture was, in fact, merely a constriction of the membranous urethra, caused by chronic spasms of the muscular fibres surrounding it, and that, a constriction of this kind, could not be distinguished from one dependent on true organic stricture." It was furthermore alleged "that the free division of one or more anterior strictures, presumed in such cases to exist, would be immediately followed by a subsidence of the spasm, permitting the easy introduction of a full-sized instrument."

Such statements, says Dr. Sands, demand the closest scrutiny and cannot be accepted without reserve." The presentment here, of my position, is certainly fair enough, with a single exception—viz, "*that a spasmodic stricture cannot be distinguished from an organic stricture.*" He should have added, *by any of the methods of diagnosis usually employed by surgeons or laid down by authorities, Dr. Sands included.* "Such statements," Dr. Sands says truly, "*demand the closest scrutiny and cannot be accepted without reserve.*" This position, is most certainly the true and scientific one, and yet, on the following page, Dr. Sands says, "Regarding the theory unsound, I cannot think the practice deduced from it, otherwise than pernicious."

Instead of giving the matter (the value of which had been attested by men equally competent, equally honest and interested in getting at the truth, as himself,) the scrutiny, he claims the subject demands, he denounces it, its originator, its advocates, its operations and its results, of whatever name or nature.

Among the various and ingenious hard sayings and judgments and

predictions, he states (and with the dogmatism which he charges upon the advocates of the reflex theory on a previous page,) that "it neglects the principal disease for one of secondary importance."

This charge is explicit and seems to invite a direct reply. Let us see—A man is suffering with recurring retention of urine and habitual difficult micturition. We are able to pass only a small instrument into his bladder; we find a contracted meatus urinarius, but the chief obstruction is in the membranous urethra. We divide the meatus urinarius—slip a large sized sound easily into the bladder; he is cured, *vide* case 1st. This case, if verified, proves the charge, that "the principle disease is neglected for one of secondary importance," to be manifestly untrue.

The case of spasmodic stricture of 17 years duration, previously referred to as an extraordinary example, proving the same thing, has been circumstantially before the profession for more than three years.

Dr. Sands attempts to evade the conclusive evidence, presented in this and kindred cases, by replying that "*such marvellous cases have not impressed me (him) with their validity.*" Now instead of the dogmatic method of disposing of this, which it is so tempting under the circumstances, I invite the "closest scrutiny" by the simple statement, that, not alone this case, but many others, and at least three of the six cases previously reported, are still within the reach of investigation. The gentleman who suffered for 17 years with retentions of urine and other difficulties which were attributed to deep organic stricture, and who was ineffectually treated for it by several distinguished surgeons in civil life, and by many others of good repute in one of the branches of our government service, is still living and well. He is both able and willing to respond to any questions as to the correctness of my statements, in regard to his troubles, and in regard to the date, mode, and permanence, of his recovery. Several of the surgeons, who treated him previously, are still living. To these, and to the gentlemen who was the subject of the trouble, I shall be glad to refer any committees of any recognised medical society, or any reputable member of our profession who has not been impressed with the validity of my statements.

Independent information, may thus be readily obtained, upon any point in connection with the alleged case.

I challenge honest criticism, or any other sort of criticism, upon every point of alleged fact connected with this case, or any other with which I have been at any time publicly or privately associated, and upon the deductions therefrom; and I hold myself in readiness to prove, by living and competent witnesses, every essential point which I have ever claimed in the matter of reflex spasmodic stricture.

Dr. Sands states, that, he has "examined many cases of well marked penile stricture without being able to discover the slightest accompanying obstruction in the membranous urethra," and that thus he feels justified in stating that the association, as alleged, of organic penile stricture with deep spasmodic stricture, is neither frequent nor *invariable*.

He would have us infer from this, that there are some who allege that such association is *invariable*. This position is a false one, as far as I am concerned, and I have never yet known it to have been alleged by any surgeon. That such association is *frequent*, I stand competent to prove. That the spasmodic character of the deeper obstruction is also frequently overlooked. That it is mistaken for and treated as organic stricture by dilatation, by internal urethrotomy and, not unfrequently, by external perineal section. In closing, Dr. Sands reports a case in which an operation was performed by one of his colleagues, in the New York Hospital, when "a stricture, 5 inches behind the meatus, was present, admitting only a filiform bougie. Anteriorly, several strictures of large calibre were diagnosed, and it was decided to divide them, in the hope that the deeper obstruction would then yield; but "after the meatus had been freely cut and the urethra so extensively divided with the dilating urethrotome, that a bulbous sound, No. 33 f. could be passed without resistance, from the meatus to the bulb, *the deep stricture remained as tight as ever.*" "The result," says Dr. Sands, "was as I had anticipated. I was greatly interested in the operation, as a scientific experiment, and have no hesitation in saying that it would have been far better if the injury inflicted on the anterior part of the urethra

had been avoided." He then denounces the operation as an "heroic procedure," and "at least useless," and "by no means free from risk."

Let us examine into this case a little. In the first place, in addition to the presence of a deep, close stricture, it had been ascertained that there were several anterior strictures of large calibre. These were first divided to 33 (the average healthy urethra being proven about $32\frac{1}{2}$ mm.) The object was, first, to clear away acknowledged pathological conditions in the anterior urethra, in order to ascertain whether or not the deeper and more important obstruction was spasmodic or organic. This was accomplished. The deep stricture remained and was thus proven to be organic, and operated on.

The experiment Dr. Sands describes as a scientific one, and yet in the next sentence he denounces it as uncalled for and unwarrantable. Why? He would probably reply because it failed. It is then unscientific to repeat a scientific experiment which has once failed? In this case, however, the *experiment* (expedient really), *did not fail*, because, through it, the true character of the deep obstruction was ascertained. The operation not only removed previously ascertained strictures, but aided efficiently in the diagnosis, which was, before this, uncertain. It is distinctly appreciated that, in many cases, true organic stricture does exist, but that it is only by first removing any presenting anterior strictures, that it can with certainty be decided whether the deep obstruction is organic or spasmodic.

Dr. Sands would have us claim that, *all deep obstructions are spasmodic*, and then, whenever he meets a case like the one cited, he can claim that there *is* such a thing as organic stricture, *therefore all strictures are organic*. Now, lest there should be further misunderstanding of this matter, I desire to state distinctly that *deep organic strictures are common*. I have reported operations upon many such, in my volume on the Radical Cure of Stricture, both by internal urethrotomy and by external perineal section. But I desire still further, as distinctly, to state that, *I have met with many more strictures which had been treated by other surgeons both by dilatation and urethrotomy external and internal, which were purely spasmodic*, and in this is the

important part of the matter. It may not be improper for me to ask why the scientific experiment, above cited, was entered upon in the New York Hospital? Dr. Sands will, in his absence, permit me to answer. Within the previous month, a patient was admitted to the wards of the New York Hospital, suffering from deep urethral stricture. The stricture was a very close one and located in the membranous urethra. The operation of perineal section was decided upon. Notices to that effect were issued. The patient, when the proper time arrived, was ætherized, brought into the amphitheatre of the New York Hospital, and the perineal section was about to be performed. The operator, a distinguished surgeon and colleague of Prof. Sands, had become familiar with my procedure in such cases, and he proposed, after ætherization, in order to test the matter of diagnosis more fully, to remove, first, several anterior contractions which were found to be present. This was accordingly done, with my dilating urethrotome, clearing the penile urethra from stricture, stopping short of the deep strictures at $5\frac{1}{2}$ inches. A large sound was then entered, and, *slipped, by its own weight, into the bladder.* A second case, in the service of the same surgeon, of exactly similar character, and two others of exactly the same kind, occurred in the service of another of Prof. Sands' colleagues, in the same hospital, within the following two months. And this it was, that led to the scientific experiment which Dr. Sands witnessed with so much interest, and the alleged failure of which, gave him so much satisfaction.

Dr. Sands, in expressing his personal feeling in regard to the operation of removing anterior strictures by dilating urethrotomy, characterizes it as a *mutilation*. Just as on a previous occasion he called a division of the meatus urinarius a *mutilation*. This appears to be a favorite expression of Dr. Sands to signify his disapprobation of a surgical procedure which he does not practice. He does not attempt the somewhat difficult task of stating the character and amount of damage done. It would be interesting to know what term he would apply to *perineal sections*, in cases of spasmodic stricture, such as was proven to exist in the four cases mentioned, and *which would have been operated on by the perineal section, if the spasmodic character of the obstruction*

had not been determined by a previous dilating urethrotomy. Dr. Sands, like all good surgeons and citizens, deprecates unnecessary and extensive cuttings as not very creditable to American surgery. No one is more likely to discredit American surgery, by unnecessary operations and by extensive and unnecessary cutting, than those who, like Dr. Sands, refuse to make use of the only means by which a certain diagnosis is possible, and who, recklessly dilate and divulse internally and even cut into the perineum, for the division of deep, close strictures, in defiance of the dangers, which the views and cases I have cited, so abundantly prove and illustrate.*

Dr. Sands claims that the theory of spasmodic stricture is unsupported by evidence derived from *pathological anatomy*. I should here like to ask Dr. Sands what he would consider the pathological anatomy of a *spasm*.

He says further, that spasmodic and organic strictures, are two "affections so widely different that they can be confounded only by an incompetent observer," and that "any doubt as to the true nature of the case can be settled by the administration of an anæsthetic."

The intended limits of this critique are already passed, but I will answer these last two allegations, which I consider untrue, by citing as proof of my position, in these and other respects, the following case in point.

Mr. D. J., planter, aged 35, was referred to me June 19, 1877, by Drs. A. Y. P. Garnet and N. S. Lincoln, of Washington, with the following history: First and only specific urethritis in 1865; severe at the outset, but soon painless, and from that time has never been quite free from a urethral discharge. Two years after, [1867,] began

* NOTE.—Within the last three months a prominent citizen of the West came to me for advice, having been treated for deep stricture by dilatation with bougies and sounds. As one of the direct results of this treatment, he had suffered with inflammation of the right testicle, which went on to suppuration, and entire loss of this organ. The same treatment continued, caused severe pains in the remaining testicle, threatening its loss also. An examination proved that *there was no deep stricture*. The only difficulty was a *contracted meatus* which had been overlooked, and on the division of which, his discharge ceased, as well as the pain in the testicle.

to appreciate a lack of force in urination with dribbling after the act. In 1867-'68-'69 was in the railroad service, which aggravated his trouble. Nothing serious, however, until 1871, when after an enforced holding of his urine, for several hours, he had an attack of retention. This, after eight hours of suffering, was reduced by the introduction of a catheter. No especial trouble again, except frequent urination, until in 1874, when, after overwork and neglect he had a second retention 12 hours—relieved by anodynes. Another a week subsequent, his physician attempted to pass catheter, but failed; bled him from the arm *ad deliquium*, when he urinated. After this, retentions were frequent, accompanied by severe vesical tenesmus, which finally produced prolapsus of rectum, great pain in region of bladder and kidneys during attacks of retention, also severe pain in the eyes, from straining. Repeated and prolonged efforts, by various medical men, to introduce a catheter, failed in every instance. Urination now every half hour and small in quantity, and inability to completely empty the bladder. This last *became much distended, and remained so*, notwithstanding frequent urination in small quantity. Suffered much from straining, in attempts to urinate, during subsequent time, up to Feb., 1877. Although repeated trials had been made by various surgeons, no instrument had been passed into the bladder since 1871, and, for previous three years, bladder habitually distended; protuberant.

At this time, a surgeon proposed to dilate his stricture, which was supposed to be in the deep urethra. No. 14, steel sound, after gentle and prolonged efforts, every morning for three weeks, preceded by a hot hip bath, was finally passed into the bladder. About a pint of urine followed the withdrawal of the sound. To this succeeded strong and painful twitchings of his limbs and severe pain in hips and over kidneys, also buttocks and thighs. This was followed, very soon, by a severe chill and fever and sweating. A similar attack of fever came on for 4 days succeeding, and he did not recover his former health for five or six weeks. After this, any unusual fatigue brought on chills. May 19, 1877, he went to Washington, and came under the charge of Dr. Garnet. A careful attempt to introduce a small

catheter failed. On the 22nd, four days after, Dr. G. associated Dr. Lincoln with him, and the patient was put under the influence of chloroform and ether and careful, persistent, trials were made with a variety of instruments to enter the bladder, all of which were resisted. The bladder was then aspirated and over a quart of urine drawn off.

On the 31st, efforts under anæsthesia were again made, for three-quarters of an hour, with result as before. Bladder again aspirated, and about same amount of urine drawn as before.

On the 5th of June another attempt under same conditions. Same result. On the 10th, again ; three pints drawn off. On the 17th, same.

Thus all efforts which appeared judicious, were made to enter the bladder, and the bladder was aspirated five times during the month. In the intervals the patient was out and able to take a little exercise, urinating every hour about a tea spoonful, sometimes with ease, at others with straining. Since August, 1876, has not been able to retain his urine when standing, and has worn a urinal habitually. Occasionally complete retention would occur, when, after application of hot cloths for a few hours, relief in the usual small degree would come. He left Washington for New York, on the 20th of June, 1877, having been last aspirated on the 17th. During his railway journey he urinated with unusual ease and freedom, but had an attack of retention on his arrival in New York, which was as usual relieved by hot cloths. This was the history given to me by the patient. He was tall, spare, with an expression of habitual suffering and irritability. Examination showed a large penis, measuring $4\frac{1}{4}$ inches in circumference ; meatus small and pouting ; bladder protuberant and dull up to within an inch of the umbilicus. No enlargement of the prostate.

Examination with the urethrometer. This was carried in to the bulbo-membranous junction and turned without discomfort up to forty. Clear to this size for three inches, then required to be turned down to twenty-eight. Three bands of stricture of 28 were recognized within an inch. The urethra was then found free from that to

within half an inch of the meatus, where it was twenty-five m.m. to the orifice. The history of the patient presented some points so similar to that of the case of chronic spasmodic stricture of seventeen years duration, (published in the ARCHIVES OF SURGERY in 1876, previously alluded to,) that I felt strongly inclined to consider the deep stricture, which was evidently in the membranous urethra, as spasmodic. I resolved to test this. I made no attempt to introduce an instrument into the bladder. Under the influence of the nitrous oxide gas, administered by my associate, Dr. Bangs, I divided the meatus urinarius to 40 F., and in order to test the influence of this procedure I did nothing else.

On the following morning, the patient announced that he had, since the operation made his water more easily than for three years, but the amount was small and the bladder was not perceptibly diminished in size. This result made me still more confident of the spasmodic nature of the deeper obstruction. On this day, June 23, 3 ½ p. m., Mr. D. J., was placed fully under the influence of ether, and with the dilating urethrotome, I divided the strictures, all of which were anterior to 4 inches, (the smallest 25 m.) up to 42 m. I then passed, what I supposed to be a 40 solid steel sound *with ease through the urethra and well into the bladder* simply by its own weight. I then passed in a very large gum catheter and drew off two pints of urine. Dr. Bangs then called my attention to the fact, that, the first instrument passed was only 36. I then took No. 40, and passed it with perfect ease well into the bladder. Slight hæmorrhage followed the operation. No chill. At one o'clock A. M., Mr. J. got up and urinated in a large stream, with complete ease, passing a full pint of urine *and completely emptying the bladder.*

From this time he had no further trouble, except the slight discomfort of urinating over the cut surface, for a few days, until it healed. At the end of a couple of weeks, he was, to all appearances, and as he said, "as well as ever in his life." He remained practically well for nearly a year, when he returned with some difficulty of micturition, but had had no retention or pain.

Examination showed a recontraction of the meatus to 34, also two bands, one at $3\frac{1}{2}$ and the other at four inches, also 34.

He was put under ether and the recontractions fully divided. An attempt to pass a full sized instrument was then made, No. 40 solid sound went easily to the bulbo membranous junction, but was arrested there. No force was used. No. 36 was then tried in the same manner gently and patiently. The same result both with and without a pressure in rectum. Then No. 30 was tried in the same way, then No. 20, then 10. Finally, down to fine filiform bougies in variety. This procedure occupied nearly an hour without success, when it was decided to make no further effort until healing of the wound had taken place, and all possible irritation, from this source had ceased.

The patient passed a good night ; no chill ; urinated three times with ease. The stream gradually decreased in force, however, for the next five days. When on Sunday, April 28, 1878, he called at my office. Urinated in my presence, in a slow, hesitating stream, but without pain. Placing him in the recumbent position on a lounge I attempted to pass a No. 5 filiform bougie. This, after a few minutes of gentle effort, slipped quickly and easily into the bladder, and then, suddenly, became *tightly hugged*. Recognizing this as a rare example of unmistakable spasmodic stricture, I at once sent for my distinguished surgical friend and neighbor, Dr. George A. Peters, to verify the correctness of my conclusions. Dr. Peters came, and appreciated the facts above stated, especially the distinct grasping of the filiform bougie by the compressor urethræ muscles. Dr. P. withdrew the bougie with some difficulty. No farther procedure was instituted. On the following day the patient complained of great nervous exhaustion, which, as he stated, came on soon after the withdrawal of the filiform, the day previous. This, however, passed off during the day, and nothing worthy of note occurred until May 4th, when the wound of operation having healed, it was decided to anæsthetise the patient, and again attempt the passage of a sound. Dr. Bangs, my associate, and Drs. J. H. Swasey and W. T. Spencer were present. After bringing the patient to unconsciousness, although some spasmodic movement of the limbs was present, I attempted to pass a

large sound. In this I failed. Smaller and smaller sizes were tried, until the small filiform, patiently used, was resisted. I then directed the patient to be put as thoroughly as possible under the influence of the anæsthetic. In about ten minutes complete muscular relaxation took place for the first time. I then again took up the solid sound, No. 38, and passed it with ease well into the bladder. This was readily followed by No. 40. Urination with ease in full stream four hours after the passage of the instruments. Day following, urinating well; feeling well; temp. $101\frac{1}{2}$. Record May 13 says "Mr. J. feels well—vesical catarrh (from which he had been suffering more or less for several weeks) declining. Makes his water readily in a full strong stream every four or five hours." Before leaving for his home Mr. J. was anxious to have another passage of the large instrument and this was done without difficulty. It was followed by a severe urethral fever however which lasted for several days, prostrating him very much, but his urination was easy and natural, not oftener than once in five or six hours, and thoroughly emptying the bladder. He gradually improved in his general health and left for Washington about the middle of May. A week after, he wrote that he had had some return of his urinary difficulty but was going South. A few months later, I received a letter from him commending a relative to my care, but not referring to his own case. Since then, although I have recently addressed a note of enquiry to him, I have not yet heard in regard to his condition.

This interesting case, appears to me to prove, not only the reality of that form of chronic spasmodic stricture which I have, (from its analogy to *vaginismus*,) venture to term "Urethrismus," but it also demonstrates its dependence upon anterior strictures, or even less prominent causes of irritation.

It demonstrates the fallacy of the claim, that spasmodic stricture may be readily distinguished from organic stricture, and that the administration of ether, necessarily causes the complete relaxation of reflex spasm. That it usually does so I admit, but in cases like that of the 17 years spasmodic stricture and the one just related, not only is this not the case, but even after the complete division of anterior

stricture, the most profound anæsthesia is required to cause it to give way.

A careful perusal of the case, will suggest several other remarkable facts, which it teaches. I have been particular in this recital, as on a previous occasion, to introduce names and dates, wherever it could with propriety be done, and would suggest that this case is still open for investigation, and that, besides the information which can be afforded by living witnesses, quite a mass of authentic documentary evidence is at the disposal of any enquiring or doubting medical man.

Dr. Sands tells us truly that "the science of medicine, although rapidly advancing, can reckon more innovations than discoveries." In closing this paper I would like to suggest, that while the above is manifestly true, yet *without innovations no discoveries are possible.*

108 West 34th st., April 18th, 1879.

